

Testimony

of

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Hearing on Perspectives on the President's Vision for Space Exploration

Committee on Science

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Abstract

President Bush's recently announced vision for a renewed program of human space exploration is examined. Budgetary requirements are considered, and specific technology development recommendations are made. Relevant policy questions are posed.

Mr. Chairman:

Thank you for inviting me to appear before the committee to discuss our nation's future in human space flight. We are at a seminal moment in discussing that future, a moment which has followed inevitably from the tragic loss of space shuttle *Columbia*, little more than a year ago. If there is a single fundamental point to be found in the report of the Columbia Accident Investigation Board – beyond identifying the technical and cultural causes of the mishap – it is that the nation's human spaceflight program has for decades lacked a unifying theme or purpose worthy of the cost and risk endemic to the enterprise. I believe it is now widely accepted that circling endlessly in low Earth orbit does not qualify as such a theme. The United States will not abandon manned spaceflight. Not to have the capability to fly humans in space, when other nations do and more will follow, is simply unacceptable for a great nation. But if we are not to abandon human spaceflight, and if our goals must reach beyond the space station, the geography of the solar system dictates the path. Of the possible venues of human activity beyond LEO, only the moon, Mars, and the nearer asteroids are within reach of the next few generations. And that is where the President's vision has directed us. It is the right path. With the remainder of this statement, I will direct my efforts to responding to the questions contained in the Committee's invitation to appear at this hearing.

Does the estimated spending through 2020 seem adequate to carry out the President's initiative? Which elements of the President's initiative seem most likely to cost more money or take more time than is currently allotted to them?

In my opinion, the issue is not whether enough money has been allocated to the President's proposed initiative, but is rather this: Why we are expecting so little for the money which has been allocated?

NASA budget estimates for the 2005-2020 period, culminating in the first manned lunar return mission by the latter year, show an aggregate allocation of some \$50-55 B (including the Crew Exploration Vehicle) to rebuild a basic "Apollo-like" capability. A top-level cost breakdown shows the following line items:

<u>Item</u>	<u>FY03 \$B</u>
Crew Exploration Vehicle	\$15
Lunar Lander	\$10-12
Launch Vehicle	\$13-16
Operations	\$9-10
<u>Other</u>	<u>\$2</u>
Total	\$50-55

This amount should be sufficient for the task as presently understood. In fact, it is possible to argue credibly that the estimate is somewhat high.

To address only one item, numerous careful studies have been performed to estimate the cost of developing a 100 metric-ton-class launch vehicle based on the use of shuttle-derived components. Such estimates consistently show non-recurring engineering development to be in the \$3-\$5B range, depending on the option considered. If other estimates show the likely development cost of a clean-sheet-of-paper design having the same payload capability to be in the \$13-16 B range, then we should seriously question whether it makes sense to pursue such an option.

The most thorough study of an "Apollo-like" return to the moon previously conducted by NASA was the "First Lunar Outpost" (FLO) effort, which occupied many of us from 1991-93. FLO was intended not as a definitive or final architecture for lunar return, but rather as a working baseline, to establish a credible point of departure for further efforts, which were unfortunately terminated at the outset of the Clinton Administration. The FLO architecture offered some improvement as compared to Apollo capability, but not so much as to be beyond our credible experience base at that time. Top level FLO cost estimates were:

<u>Item</u>	<u>FY92 \$B</u>
Crew Vehicle Development & 1 st Unit	\$7.4
Surface Habitat and Systems	\$2
Launch Vehicle Development & 3 Vehicles	\$12.6
<u>Unmanned Lander & Cargo Production (2 Units)</u>	<u>\$3</u>
Total	\$25

The FLO costs must be inflated by about 30% to account for the difference between 1992 and 2003 dollars, resulting in an estimate of about \$33 B for an initial lunar return. Also, the FLO studies assumed that the then-planned International Space Station habitat module would be available (with some modifications) for use on the lunar surface. Substantial development resources would be required to restore such a capability at this point, were it to be included in a lunar return mission. However, because a surface habitat is not included in the current planning estimate, it should be deleted from the comparison, yielding a 2003 FLO cost estimate of about \$30 B, no more than 60% of NASA's current assessment.

Considerable study was devoted to FLO cost and feasibility analysis, in some cases by the same NASA personnel as are engaged in the present effort. It is difficult to understand why there should exist such a discrepancy between today's estimates and those of a decade ago. One can certainly understand that any estimate derived from a design study will lack the credibility of a completed development program. But it is difficult to understand why two estimates for very similar development programs would differ so greatly.

Additional perspective can be gained by noting that the cost of the entire Apollo program was about \$130 B in today's dollars. This included massive technology and infrastructure development, as well as the operational cost of eleven manned missions, including six lunar landings. It does not seem reasonable that 40% or more of this figure should be required to execute a single mission of a similar class today.

For advocates of spaceflight, including myself, more money is always better, and is certainly preferable to less money! But I would submit that our first order of business is to examine our culture, the aerospace culture, and ourselves, to understand why we believe it costs so very much more to operate in space than to perform almost any other human activity.

NASA's spending plan through 2020 does not explicitly include any activity in support of manned missions to Mars, or indeed any exploration activity beyond early lunar return. I therefore cannot comment on the reasonableness of such plans at this time. This is regrettable, because the goal of pushing on to Mars should, in part, drive program requirements even while planning to return to the moon.

What are the greatest technological hurdles the President's initiative must clear to be successful? To what extent must resolving some technological issues await further fundamental research? For example, how much work on a spacecraft for a Mars mission can be done before more is known about the effect on humans of spending long periods of time in space? How much work can be done before new propulsion technologies are developed?

The question of what technological hurdles stand between us and the fulfillment of the President's vision depends, to a very great extent, on the mission architecture(s) which are selected to achieve that vision. In a very real sense, there is essentially no fundamental new technology required to enable human return to the moon, the establishment of a lunar base, or the first voyages to Mars.

It is true that technical challenges exist, and that there are numerous systems needed to implement the vision that are not currently in production. Among the specific engineering development tasks needing to be performed are:

- NASA should initiate development of a heavy lift launch vehicle having a payload capacity of at least 100 metric tons to low Earth orbit (LEO). Such a vehicle is the single most important physical asset enabling human exploration of the solar system. The use of shuttle-derived systems offers what is quite likely to be the most cost-effective near-term approach.
- Much cargo (including humans) does *not* need to be launched in very large packages. We desperately need much more cost effective Earth-to-LEO transportation for payloads in the size range from a few thousand to a few tens of thousands of pounds. In my judgment, this is our most pressing need, for it controls a major portion of the cost of everything else that we do in space. Yet, no active U.S. government program of which I am aware has this as its goal. Again, shuttle-derived systems, particularly emphasizing use of the RSRB, may offer a useful approach.
- New propulsion systems are unnecessary. We can certainly return to the moon or go to Mars using existing chemical propulsion systems. Looking ahead, development of nuclear propulsion should be re-initiated to allow more efficient travel beyond cislunar space, but such systems are not altogether new. The NERVA (nuclear engine for rocket vehicle applications) program produced a working nuclear upper-stage engine and demonstrated excellent performance in extensive ground tests, before regrettably being cancelled in 1973.
- Compact space qualified nuclear power systems are required for extended human presence on the Moon and Mars.
- The efficient establishment of permanent human bases on the Moon, Mars, and certain asteroids requires the use of *in situ* resources to minimize the amount of material and equipment which must be brought from Earth. The technology for such exploitation has yet to be fully developed, though promising experiments have been conducted.
- Space and planetary surface habitat and suit technology is at present insufficient for the needs of an extended program of human space exploration. Improvements in suit technology are of the highest priority.

Physiological challenges also exist. We have considerable experience in the microgravity environment, and some practical and effective countermeasures have shown promise in minimizing bone loss, though more work is clearly needed. However, in the near term it is very clear from the existing base of human spaceflight experience that microgravity effects are not an impediment to lunar return or to expeditions to Mars. And, as a practical matter, it is always possible to design our spaceships to supply artificial gravity by spinning them to generate the necessary centrifugal force.

The long-term human adaptation to life on other planetary surfaces is another matter. We have at present no clear understanding of how the human organism will respond to fractional gravitational environments such as will be experienced on the moon and Mars.

Overall, however, the most difficult physiological issue is likely to be that of cosmic heavy-ion radiation. The human effects of and countermeasures for heavy ion radiation, encountered in deep space but not in the LEO environment of the ISS, have received little attention thus far.

These are the essential technical and physiological challenges as I see them. Exploration missions will not be accomplished without human risk. While certainly worthy of our attention, however, none of these is so daunting that we should stay home.

However, it is always possible to make the problem more difficult. Some of the spaceflight architectures that have been advocated seem intended to stun the observer with sheer complexity. If we are planning to defer return to the moon until we have established L1 Gateways, solar electric propulsion systems to ferry liquid oxygen up from low Earth orbit, and so on, then it may indeed be possible to spend a very large amount of time and money on technology development. I do not recommend that we pursue such paths.

Are the International Space Station and the moon the most appropriate stepping stones for human space exploration if the ultimate objective is a human landing on Mars? What would be the advantages and disadvantages of a program that was targeted instead directly on sending a human to Mars?

Given that ISS is to be completed, there are specific tasks associated with going to Mars for which it can be useful. Certainly, it can be useful in carrying out controlled experiments to study the effects of microgravity, and proposed countermeasures, on humans, provided of course that it is equipped with a habitat module or modules. It can serve as an aid to crew training, acclimating a proposed Mars crew, or extended-duration lunar crew, to the regimen of spaceflight in company with each other. It can serve as a testbed for the space qualification of specific systems, or even vehicles, prior to their use on extended voyages far from home. In a word, ISS can help us learn to live and work in space.

But the more important question is whether the return to be obtained from the use of ISS to support exploration objectives is worth the money yet to be invested in its completion. The nation, through the NASA budget, plans to allocate \$32 B to ISS (including ISS transport) through 2016, and another \$28 B to shuttle operations through 2011. This total of \$60 B is significantly higher than NASA's current allocation for human lunar return. It is beyond reason to believe that ISS can help to fulfill any objective, or set of objectives, for space exploration that would be worth the \$60 B remaining to be invested in the program.

Equally important is the delay in pursuing the President's vision. Respecting present budget constraints, we return to the moon in 2020, thus accomplishing in 16 years what it required eight years to achieve in the 1960s. This is not because the task is so much more difficult, or because we are today so much less capable than our predecessors, but because we do not actually begin

work on the task until 2011. I do not need to point out to this body the political pitfalls endemic to such a plan.

I, and others, have elsewhere advocated that the shuttle should be returned to flight and the ISS brought to completion, if only because the program's two-decade advocacy by the United States and commitment to its international partners should not be cavalierly abandoned. But, if there is no additional money to be allocated to space exploration, this position becomes increasingly difficult to justify. It is worth asking whether our international partners might judge the issue similarly.

With regard to the moon, I believe the experience to be gained by living on and exploring another planetary surface only a few days away from home will be invaluable to the successful conduct of a future Mars expedition. Certainly such experience is not essential; one can readily envision a Mars expedition architecture which does not employ any further lunar experience as a stepping stone. But because it can be envisioned does not make it wise. I personally consider it an act of technological hubris to proceed directly to Mars, with no human experience beyond Earth orbit having been incurred since 1972. It can be done, and it will be cheaper, but the risk to both the mission goals and to human life will be significantly higher.

If the goal of the United States is solely to mount an expedition to Mars, then I can at least understand, if not credit, the concern that returning to the moon is a distraction. But if the goal of the United States is to be truly a spacefaring nation, then bypassing the moon is silly.

What questions is it most important for Congress to ask as it evaluates the proposed initiative?

In discussing the President's initiative as it has been put before us, we in the space policy community have spent most of our time debating the cost and technical merit of one approach or the other; whether it makes sense to go to the moon or not; if so, what to do and how much time to spend there; what new technology is or is not needed, and why, and so on. These are of course interesting questions – but they are not in my opinion the questions which are most relevant for the Congress to ask. Among these more relevant questions might be the following:

- Why does spaceflight – human or robotic – cost so much more than other comparably complex human activities, and what can be done to remedy the situation?
- Is a serious program of human space exploration sustainable, given the “cost of doing business” presently associated with the enterprise?
- What incentives can be offered to proven and well-established aerospace contractors to devise innovative and cost-effective, yet safe and reliable, approaches to building a new human spaceflight infrastructure?
- Where and how does NASA intend to engage the entrepreneurial high-tech culture which has made our nation the envy of so many others, in so many areas other than aerospace? What can we do to bring the engine of capitalism to spaceflight?
- What is the proper role of prizes, or of pay-for-performance contracts, in stimulating and encouraging the high-tech community to devote its attention to aerospace?

- Can or should the Congress establish prizes for specific accomplishments in spaceflight, independently of NASA?
- What is NASA's proper role in the development of new space systems, beyond setting requirements to be met through competition in industry?
- What is NASA's proper role, as an agency of the U.S. government, in the conduct of future spaceflight operations?
- If the exploration of new worlds requires technologies and skills beyond those presently available within NASA – and it clearly does – how are the skills of other agencies and laboratories to be used effectively in the service of the larger mission? How will the overall effort be directed?
- Given that we as a nation will spend a certain amount each year on civil space activities, what would Americans prefer to see this money used for? What vision for space exploration excites people enough to cause them to believe that the money they spend on it is well spent? Can a reasonable consensus even be found? How do we know?
- Is the United States interested in leading an international program of space exploration? Which nations might be competitors, and which might be partners? How and in what role do we view our potential partners in the enterprise? What do our potential partners think about this? How do we know?

Witness Biography

Michael D. Griffin is currently President and Chief Operating Officer of In-Q-Tel. On March 29th, he will succeed to his new position as Space Department Head at the Johns Hopkins University Applied Physics Laboratory.

Prior to joining In-Q-Tel, Mike was CEO of the Magellan Systems Division of Orbital Sciences Corporation. He also served as General Manager of Orbital's Space Systems Group and as the company's Executive Vice President/Chief Technical Officer. He has previously served as both the Chief Engineer and the Associate Administrator for Exploration at NASA, and as the Deputy for Technology of the Strategic Defense Initiative Organization.

Before joining SDIO in an executive capacity, Mike played a key role in conceiving and directing several "first of a kind" space tests in support of strategic defense research, development, and flight testing. These included the first space-to-space intercept of a ballistic missile in powered flight, the first broad-spectrum spaceborne reconnaissance of targets and decoys in midcourse flight, and the first space-to-ground reconnaissance of ballistic missiles during the boost phase.

Mike holds seven degrees in the fields of Physics, Electrical Engineering, Aerospace Engineering, Civil Engineering, and Business Administration, has been an Adjunct Professor at the George Washington University, the Johns Hopkins University, and the University of Maryland, and is the author of over two dozen technical papers and the textbook *Space Vehicle Design*. He is a recipient of the NASA Exceptional Achievement Medal, the AIAA Space Systems Medal, the DoD Distinguished Public Service Medal, and is a Fellow of the AIAA and the AAS. Mike is a Registered Professional Engineer in Maryland and California, and a Certified Flight Instructor with instrument and multiengine ratings.